the remains of several large yellow frogs—and adds, " Still more difficult to credit, eels are often found among the food-store. How he catches *them* I have some curiosity to know. Yarrell says eels slide like serpents over the dewy grass from one drain to another. If so, the difficulty ends ; for the foumart's instinct would soon teach it to watch for the land progress of its slimy prey."

In dear old Bewick, the delight of our youth, the woodcut of the foumart represents the animal with an eel in its mouth, the accuracy of the illustration being founded on the fact that several fine eels were discovered in its retreat, and that it had been tracked in the snow to the banks of a rivulet. Now we have never met with any one who could assert that they had ever seen a foumart in the water, and the matter has always been a puzzle to us ; but we have to thank Mr. Colquhoun for dispelling the mystery.

We regret that the limited space at our disposal forbids us to indulge in further quotations. Suffice it to say that the badger (*Meles taxus*), the otter (*Lutra vulgaris*), and even the rat, all come in for their share of notice, their habits being graphically described and illustrated by characteristic anecdotes. We cordially recommend this interesting essay to the general reader as well as to the naturalist and sportsman.

PROCEEDINGS OF LEARNED SOCIETIES.

ROYAL SOCIETY.

Jan. 30, 1873.—George Busk, Esq., Vice-President, in the Chair.

"Note on the Origin of *Bacteria*, and on their Relation to the Process of Putrefaction." By H. CHARLTON BASTIAN, M.D., F.R.S.

In his now celebrated memoir of 1862, M. Pasteur asserted and claimed to have proved (1) that the putrefaction occurring in certain previously boiled fluids after exposure to the air was due to the contamination of the fluids by *Bacteria*, or their germs, which had before existed in the atmosphere, and (2) that all the organisms found in such fluids have been derived more or less immediately from the reproduction of germs which formerly existed in the atmosphere.

The results of a long series of experiments have convinced me that both these views are untenable.

In the first place, it can be easily shown that living *Bacteria*, or their germs, exist very sparingly in the atmosphere, and that solutions capable of putrefying are not commonly infected from this source.

It has now been very definitely ascertained that certain fluids exist which, after they have been boiled, are incapable of giving birth to *Bacteria*, although they continue to be quite suitable for

the support and active multiplication of any such organisms as
may have been purposely added to them.   Amongst such fluids I
may name that now commonly known as " Pasteur's solution," and
also one which I have myself more commonly used, consisting of
a simple aqueous solution of neutral ammonic tartrate and neutral
sodic sulphate *.   When portions of either of these fluids are
boiled and poured into superheated flasks, they will continue quite
clear for many days, or even for weeks ; that is to say, although
the short and rather narrow neck of the flask remains open the
fluids will not become turbid, and no *Bacteria* are to be discovered
when they are submitted to microscopical examination.

But in order to show that such fluids are still thoroughly
favourable media for the multiplication of *Bacteria*, all that is
necessary is to bring either of them into contact with a glass rod
previously dipped into a fluid containing such organisms.   In about
thirty-six hours after this has been done (the temperature being
about 80° F.), the fluid, which had hitherto remained clear, becomes
quite turbid, and is found, on examination with the microscope, to
be swarming with *Bacteria*†.

Facts of the same kind have also been shown by Dr. Burdon
Sanderson‡ to hold good for portions of boiled " Pasteur's solu-
tion."   Air was even drawn through such a fluid daily for a time,
and yet it continued free from *Bacteria*.

Evidence of this kind has already been widely accepted as justify-
ing the conclusion that living *Bacteria* or their germs are either
wholly absent from or, at most, only very sparingly distributed
through the atmosphere.   The danger of infection from the
atmosphere having thus been got rid of and shown to be delusive,
I am now able to bring forward other evidence tending to show
that the first *Bacteria* which appear in many boiled infusions
(when they subsequently undergo putrefactive changes) are evolved
de novo in the fluids themselves.   These experiments are more-
over so simple, and may be so easily repeated, that the evidence
which they are capable of supplying lies within the reach of all.

That boiling the experimental fluid destroys the life of any
Bacteria or *Bacteria*-germs preexisting therein is now almost uni-
versally admitted ; it may, moreover, be easily demonstrated.   If
a portion of " Pasteur's solution " be purposely infected with living
Bacteria and subsequently boiled for two or three minutes, it will
continue (if left in the same flask) clear for an indefinite period ;
whilst a similarly infected portion of the same fluid, not subse-
quently boiled, will rapidly become turbid.   Precisely similar
phenomena occur when we operate with the neutral fluid which
I have previously mentioned ; and yet M. Pasteur has ventured to
assert that the germs of *Bacteria* are not destroyed in neutral or

* In the proportion of 10 grains of the former and 3 of the latter to 1 ounce
of distilled water.

† The Modes of Origin of the Lowest Organisms, 1871, pp. 30, 51.

‡ Thirteenth Report of the Medical Officer of the Privy Council (1871),
p. 59.

slightly alkaline fluids which have been merely raised to the boiling-point*.

Even M. Pasteur, however, admits that the germs of *Bacteria* and other allied organisms are killed in slightly acid fluids which have been boiled for a few minutes ; so that there is a perfect unanimity of opinion (amongst those best qualified to judge) as to the destructive effects of a heat of 212° F. upon any *Bacteria* or *Bacteria*-germs which such fluids may contain.

Taking such a fluid, therefore, in the form of a strong filtered infusion of turnip, we may place it after ebullition in a superheated flask with the assurance that it contains no living organisms. Having ascertained also by our previous experiments with the boiled saline fluids that there is no danger of infection by *Bacteria* from the atmosphere, we may leave the rather narrow mouth of the flask open, as we did in these experiments.   But when this is done, the previously clear turnip-infusion invariably becomes turbid in one or two days (the temperature being about 70° F.), owing to the presence of myriads of *Bacteria*.

Thus, if we take two similar flasks, one of which contains a boiled " Pasteur's solution " and the other a boiled turnip-infusion, and if we place them beneath the same bell-jar, it will be found that the first fluid remains clear and free from *Bacteria* for an indefinite period, whilst the second invariably becomes turbid in one or two days.

What is the explanation of these discordant results ?   We have a right to infer that all preexisting life has been destroyed in each of the fluids† ; we have proved also that such fluids are not usually infected by *Bacteria* derived from the air ; in this very case, in fact, the putrescible saline fluid remains pure, although the organic infusion standing by its side rapidly putrefies.   We can only infer, therefore, that whilst the boiled saline solution is quite incapable of engendering *Bacteria*‡, such organisms are able to arise *de novo* in the boiled organic infusion.

Although this inference may be legitimately drawn from such experiments as I have here referred to, fortunately it is confirmed and strengthened by the labours of many investigators who have worked under the influence of much more stringent conditions, and in which closed vessels of various kinds have been employed §.

Whilst we may therefore infer (1) that the putrefaction which

* How unwarrantable such a conclusion appears to be, I have elsewhere endeavoured to show. See ' Beginnings of Life,' 1872, vol. i. pp. 326–333, 372–399.

† [*Note.* Jan. 31, 1873.]—In ' The Beginnings of Life,' vol. i. p. 332, note 1, I have cited facts strongly tending to show that *Bacteria* are killed in infusions of turnip or of hay when these have been heated to a temperature of 140° F. They also seem to die at the same temperature in solutions of ammonic tartrate with sodic phosphate.

‡ See ' Beginnings of Life,' vol. ii. p. 35, and vol. i. p. 463.

§ See a recent communication by Prof. Burdon Sanderson, in ' Nature,' January 9th.

occurs in many previously boiled fluids when exposed to the air is not due to a contamination by germs derived from the atmosphere, we have also the same right to conclude (2) that in many cases the first organisms which appear in such fluids have arisen *de novo*, rather than by any process of reproduction from pre-existing forms of life.

Admitting, therefore, that *Bacteria* are ferments capable of initiating putrefactive changes, I am a firm believer also in the existence of not-living ferments under the influence of which putrefactive changes may be initiated in certain fluids—changes which are almost invariably accompanied by a new birth of living particles capable of rapidly developing into *Bacteria*.

Feb. 27, 1873.—William Spottiswoode, M.A., Treasurer and Vice-President, in the Chair.

" On Leaf-Arrangement." By HUBERT AIRY, M.A., M.D.

Assuming as generally known the main facts of leaf-arrangement, the division into the whorled and spiral types, and in the latter more especially the establishment of the convergent series of fractions $\frac{1}{2}$, $\frac{1}{3}$, $\frac{2}{5}$, $\frac{3}{8}$, $\frac{5}{13}$, $\frac{8}{21}$, $\frac{13}{34}$, $\frac{21}{55}$, $\frac{34}{89}$, $\frac{55}{144}$, &c. as representatives of a corresponding series of spiral leaf-orders among plants, we have to ask, what is the meaning that lies hidden in this law?

Mr. Darwin has taught us to regard the different species of plants as descended from some common ancestor ; and therefore we must suppose that the different leaf-orders now existing have been derived by different degrees of modification from some common ancestral leaf-order.

One spiral order may be made to pass into another by a twist of the axis that carries the leaves. This fact indicates the way in which all the spiral orders may have been derived from one original order, namely by means of different degrees of twist in the axis.

We naturally look to the simplest of existing leaf-orders, the two-ranked alternate order $\frac{1}{2}$, as standing nearest to the original ; for it is manifest that the orders at the other extreme of the series (the condensed arrangement of scales on fir-cones, of florets in heads of *Compositæ*, of leaves in close-lying plantains, &c.) are special and highly developed instances, to meet special needs of protection and congregation : they are, without doubt, the latest feat of phyllotactic development ; and we may be sure that the course of change has been from the simple to the complex, not the reverse. This point will be illustrated by experiment below.

But first, what are the uses of these orders? and at what period of the leaf's life does the advantage of leaf-order operate? The period must be that at which the leaf-order is most perfect— not, therefore, when the twig is mature, with long internodes between the leaves, but while the twig and its leaves are yet *in the bud* ; for it is in the bud (and similar crowded forms) that

the leaf-order is in perfection, undisturbed by contortions or inequalities of growth; but as the bud develops into the twig the leaves become separated, the stem often gets a twist, the leaf-stalks are curved and wrung to present the blades favourably to the light, and thus the leaf-order that was perfect in the bud is disguised in the grown twig.

In lateral shoots of *yew* and *box* and *silver fir* we see how leaves will get their stalks twisted to obtain more favourable exposure to light; and if general distribution round the stem were useful to the adult leaves, we should expect the leaves of a vertical *elm*-shoot (for example) to secure such distribution by various twists of stalk and stem; but the leaf-blades of the elm keep their two ranks with very great regularity. This goes to show that it is not in the mature twig that the leaf-order is specially advantageous.

In the *bud* we see at once what must be the use of leaf-order. It is for *economy of space*, whereby the bud is enabled to retire into itself and present the least surface to outward danger and vicissitudes of temperature. The fact that the order $\frac{1}{2}$ does not exhibit this advantage in any marked degree, supports the idea that this order is the original from which all the more complex spiral orders have been derived.

The long duration of the bud-life as compared with the open-air life of the leaf gives importance to the conditions of the former. The open-air life of the bud is twelve months, and adding the embryo life of the bud, we have about a year and a half for the whole life of the bud; and for the twelve months of its open-air life it is in a state of siege, against which a compact arrangement of its embryo-leaves within must be of great value. But the open-air life of the unfolded leaves is (except in evergreens) not more than six months.

That the order $\frac{1}{2}$ would under different degrees of contraction (with twist) assume successively the various spiral orders that exist in nature, in the order of their complexity, $\frac{1}{3}$, $\frac{2}{5}$, $\frac{3}{8}$, $\frac{5}{13}$, &c., may be shown by the following experiment :—

Take a number of spheres (say oak-galls) to represent embryo leaves, and attach them in two rows in alternate order ($\frac{1}{2}$) along opposite sides of a stretched india-rubber band. Give the band a slight twist, to determine the direction of twist in the subsequent contraction, and then relax tension. The two rows of spheres will roll up with a strong twist into a tight complex order, which, if the spheres are attached in close contact with the axis, will be nearly the order $\frac{1}{3}$, with three steep spirals. If the spheres are set a little away from the axis, the order becomes condensed into (nearly) $\frac{2}{5}$, with great precision and stability. And it appears that further contraction, with increased distance of the spheres from the axis, will necessarily produce the orders (nearly) $\frac{3}{8}$, $\frac{5}{13}$, $\frac{8}{21}$, &c. in succession, and that these successive orders represent successive *maxima* of stability in the process of change from the simple to the complex.

It also appears that the necessary sequence of these successive steps of condensation, thus determined by the geometry of the case, does necessarily exclude the non-existent orders, $\frac{1}{4}$, $\frac{2}{7}$, $\frac{3}{7}$, $\frac{4}{9}$, $\frac{4}{11}$, &c.

Numbering the spheres from 0 upwards, it appears that, under contraction, the following numbers are brought successively into contact with 0, alternately to right and left :—1, 2, 3, 5, 8, 13, 21, 34, 55, 89, 144, &c. None of them stands vertically above 0 while in contact with it, but a little to the right or a little to the left ; and so far the results of this experiment fall short of the perfect fractions $\frac{1}{3}$, $\frac{2}{5}$, $\frac{3}{8}$, $\frac{5}{15}$ &c. : but in this very failure the results of the experiment are more closely in agreement with nature than are those perfect fractions themselves ; for those fractions give the angular divergence only in round numbers (so to speak), and lose account of the little more, or the little less, which makes all the difference between a vertical rank and a spiral. In the large majority of spiral-leaved plants, one has to be content with " $\frac{2}{5}$ nearly " or " $\frac{3}{8}$ nearly ;" and it is difficult to find a specimen in which the fraction represents the order exactly.

The geometrical relations of the members of the above series 1, 2, 3, 5, 8, 13, &c. are as simple as their numerical relations.

Analysis of the order seen in the head of the sunflower and other examples, by consideration of their several sets of spirals, presents a striking agreement with the above synthetical process. In the sunflower, a marginal seed taken as 0 is found to be in contact with the 34th, the 55th, and the 89th (counted in order of growth), and even with the 144th, if there is not contact with the 34th. The dandelion, with a lower degree of conden- sation, has 0 in contact with the 13th, the 21st, and the 34th in large specimens ; the house-leek in its leaf-order has 0 in con- tact with the 5th, 8th, and 13th ; the apple-bud has 0 in contact with the 2nd, 3rd, and 5th ; and thus we see that in nature the very same series of numbers is found to have contact-relation with 0 which we have already seen possessing that relation in the experimental condensation of the order $\frac{1}{2}$.

Difference of leaf-order in closely allied species (e. g. *Plantago major* and *P. coronopus*) is found in close relation to their different habits and needs.

The prevalence of the order $\frac{1}{2}$ in marine *Algæ*, and in *Gramineæ*, a low-developed gregarious group, and its singular freedom from individual variation in that group and in elm, beech, &c., support the view that this order is the original of the spiral orders.

In many plants we find actual transition from the order $\frac{1}{2}$ to an order more complex, as, for instance, in *Spanish chestnut, laurels, nut, ivy* ; and these instances agree in presenting the complex order in the buds that occupy the most exposed situations, while they retain the simple $\frac{1}{2}$ in the less-exposed lateral buds. Several kinds of *aloe* have the order $\frac{1}{2}$ in their basal leaves, and a higher order in the remainder. A species of *cactus* often contains a

complete epitome of phyllotaxy in a single plant, or even in a single shoot.

Shoots of *acacia* often present a zigzag disposition of their leaves, on either side of the branch, which seems unintelligible except as a distortion of an original two-ranked order.

The prevalent two-ranked arrangement of rootlets or roots seems to be a survival underground of an order which originally prevailed through the whole plant, root, stem, and branch.

In the whole Monocotyledonous class the first leaves in the seed have the order $\frac{1}{2}$.

In the Dicotyledonous class the first leaves in the seed have the simplest order of the whorled type.

As the spiral orders have probably been derived from a two-ranked alternate arrangement, so the whorled orders have probably been derived from a two-ranked *collateral* (two abreast) arrangement. This is illustrated by an experiment similar to the former ; and it is seen that successive parallel horizontal pairs of spheres are compelled under contraction to take position at right angles to one another, exactly in the well-known crucial or decussate order. These whorls of two contain potentially whorls of three and four, as is seen in variations of the same plant ; but the experiment does not show the change.

The reason of the non-survival of the (supposed) two-ranked *collateral* order lies in its manifest instability ; for under lateral pressure it would assume the alternate, and under vertical the crucial order.

The bud presents in its shape a state of equilibrium between a force of contraction, a force of constriction, and a force of growth.

To sum up, we are led to suppose that the original of all existing leaf-orders was a two-ranked arrangement, somewhat irregular, admitting of two regular modifications, the alternate and the collateral—and that the alternate has given rise to all the spiral orders, and the collateral to all the whorled orders, by means of advantageous condensation in the course of ages.

March 6, 1873.—Sir George Biddell Airy, K.C.B., President, in the Chair.

"On a new Genus of Amphipod Crustaceans." By RUDOLPH VON WILLEMÖES-SUHM, Ph.D., Naturalist to the 'Challenger' Exploring-Expedition.

In lat. 35° 47′, long. 8° 23′, off Cape St. Vincent, the trawl was sent down to a depth of 1090 fathoms on the 28th of January, and brought up, among other very interesting things, a large transparent Amphipod with enormous faceted eyes. The animal, evidently hitherto unknown, will be the type of a new genus, having the following characters :—

Thaumops, nov. gen.

Caput oblongum, inflatum, oculis maximis superiorem capitis partem tegentibus. Segmenta thoracica 6, abdominalia 5. Antennarum in feminis par unum, maxillarum par unum, pedum
paria duo minima maxillarum locum tenentia. Mandibulæ
nullæ. Pedes thoracici 5, abdominales 3 in utroque latere.
Appendices caudales 4. Gangliorum pectoralium paria 5,
abdominalium 3.

Thaumops pellucida, n. sp.

Corpus longitudine 14 mm., latitudine 21 mm., pellucidum.

An anatomical description of this interesting animal is given,
illustrated by two plates; and it is shown that, among the
Amphipods known to us, *Phronima* is its nearest relative. But
there are so many points in which this genus differs from *Phronima*,
that it cannot form a member of the family Phronimidæ; and
I therefore propose to establish for it a new family, Thaumopidæ,
belonging to the tribe of *Hyperina*.

The form of the *head* is totally different from that of *Phronima*;
the antennæ are not situated near the mouth, but at its front;
and the enormous faceted eyes occupy its upper surface. The
first two pairs of thoracic appendages are not, as in *Phronima*,
ambulatory legs, but maxillipeds, so that only five pairs of legs
are ambulatory in *Thaumops*. The *thorax* is composed of six
segments—the first of which has, on its underside, the vulva
and one pair of maxillipeds; and the second, representing two
segments, bears two pairs of appendages, the larger maxillipeds
and the first pair of ambulatory legs. The *abdomen* consists of
five segments, with three pairs of pedes spurii, the caudal appendages being attached to the fourth and fifth segments.

The animal being beautifully transparent, the *nervous system*
could be carefully worked out without dissecting it; the position
of the nerves going out from the cephalic ganglion, as well as
that of the five pairs of thoracic and the three pairs of abdominal
ganglia, could be ascertained. The *eyes*, having at their borders
very peculiar appendages, were examined; and a description is
given of the structure of the large crystalline bodies which are
to be seen in them. Organs of hearing and touch have not been
discovered.

The *mouth* is covered by a pair of maxillæ and a small labium.
There is a recurved œsophageal passage leading into a large
cæcal stomach, and an intestinal tube departing from near the end
of the œsophagus and running straight to the anus.

The *heart* is an elongated tube extending from the second to
the fifth segment, with probably three openings. Three pairs of
transparent sac-like gills are attached at the base of the second,
third, and fourth pairs of feet.

Genital organs.—The single specimen taken is a female. The
ovary, probably composed of two ovaries, has a rose-colour; and

the genital papilla is situated at the under part of the first segment; it is covered by two small lamellæ, which in this case did not sustain the eggs, which were found to be attached to the first pair of ambulatory legs. The animal seems to carry them in a similar manner as the pycnogonid *Nymphon*.

Development.—The eggs contained embryos having already the antennæ, the five pairs of legs, and the abdominal feet; they show that *Thaumops* has to undergo no metamorphosis, and that the young ones leave the eggs with all their appendages well developed.

Mode of life.—It could not be made out whether *T. pellucida* inhabits the deep sea, or whether it is, like *Phronima*, a pelagic animal, having been caught by the trawl only as the latter came up from the depths.

H.M.S. 'Challenger,' Teneriffe,
    February 13, 1873.

March 20, 1873.—Mr. George Busk, Vice-President, in the Chair.

"On the Distribution of the Invertebrata in relation to the Theory of Evolution." By JOHN D. MACDONALD, M.D., F.R.S., Staff Surgeon R.N., Assistant Professor of Naval Hygiene, Netley Medical School.

All organized beings exhibit both structural and functional conditions, forming the grounds of comparison by which natural affinities in smaller groups, and points of difference in larger ones, are detected and established in systematic classification.

General anatomical or physiological considerations in agreement are usually of more importance than the harmony of single or special conditions of either description; and though structural characters, as a rule, are superior to those of a functional nature, much may be learnt from an arrangement founded on physiological principles alone. I have elsewhere pointed out the deceptiveness of taking the habit of life as a guide in classification, though this is adopted by many zoologists; for essentially different types may live under precisely similar circumstances, or the habit of life may be very different in the members of the same type. Thus, if we look upon a pectinate gill for aquatic respiration, fluviatile or marine, and the amphibious coincidence of this with a pulmonary chamber, or the presence of the latter cavity alone in purely terrestrial Gasteropods, as grouping characters, nothing can be more erroneous; for all these conditions of the respiratory system are to be met with in unequivocal examples of the same group, anatomically defined, as in the Nerite alliance, or that of *Rissoa* for example. Nevertheless animals so simple in their nature as the Protozoa may be distributed physiologically, with some show of truthfulness in the resulting scheme.

Passing the leading types of the Protozoa in review, we notice that the Gregarinidæ alone are essentially parasitic in their habit of life, obtaining nutriment from materials elaborated by

other animals. All the rest are therefore non-parasitic, deriving their sustenance from the outer world. If we now consider the manner in which nutritious matters are taken up and assimilated by these animals, we find that some of them must subsist on organic substances in solution, which are absorbed by the general surface of the body. Moreover we observe that this takes place either indirectly through a more or less consistent investing substance, or directly through the pores, foramina, or fenestrations of the calcareous or siliceous capsules protecting the contained sarcode bodies. In other instances, on the contrary, solid food is actually consumed by mouthless beings, which simply open their bodies to receive it; and this opening of the body may take place at any part of the surface most convenient, or it may be restricted to a definite locality, shadowing forth the permanent mouth of the Stomatoda, or even that of the most primitive form of Hydrozoa.

The annexed Table of arrangement is drawn up in accordance with the foregoing remarks.

Physiological Classification of the Protozoa.

Habit of life and mode of nutrition :—

I. Parasitic	*Gregarinidæ.*
II. Non-parasitic.	
A. Assuming food in a state of solution by absorption of the general surface.	
1. Indirectly through a medium	
a. Forming a cell-like envelope	*Thalassicollidæ.*
b. Lining porous canals in the common mass ...	*Porifera.*
2. Directly through	
a. The pores or foramina of a calcareous shell ...	*Foraminifera.*
b. Fenestrations of a siliceous shell..................	*Polycystina.*
c. A more largely exposed surface	*Acanthometridæ.*
B. Assuming solid food by an adventitious mouth.	
1. At any part of the surface where the contact is made	*Monera, Amœba, &c.*
2. At a definite part, determined by the opening of the shell	*Gromia, Difflugia, &c.*
C. Assuming solid food by a permanent mouth,	
1. The same orifice being also excretory..............	*Infusoria.*
2. Discharging excreta by a rudimentary anus	*Noctilucidæ.*

This Table may be said to afford us good general grounds for forming an estimate of the relative superiority of the several types thus physiologically defined, and it is mainly in keeping with their more commonly received distribution founded on structural particulars.

A show of progressive improvement is seen in the respective sections A, B, and C—though to all appearance the simplest group of animals in existence, namely the *Monera* of Häckel, is included in the section B. These rudimentary creatures are destitute of both nucleus and contractile vesicle, though exhibiting activities in movement, taking food, and reproducing their kind, not even second to those of *Amœba* and its allies. The smallest ciliated molecule endowed with animal life could not present a

more simple structure than that of the perfectly homogeneous and jelly-like *Monera*. Indeed the evolution of any of the other primitive forms from a plastic source like this is quite conceivable, though of course we have no actual means of observing such a transmutation.

Moreover the development of amoeboids in some part of the life-history of most Protozoa would appear to stamp that form as the earliest genetic type of beings. With the exception of a nucleus and a contractile vesicle, *Amoeba* itself may have sprung from *Protamoeba*; and the finally encysted jelly-globules of *Protomyxa* and *Myxastrum* breaking up into naked amoeboids, or pseudonavicellæ liberating them, very strikingly suggest the source from which the *Gregarinæ* may have been evolved.

The valuable researches of Mr. Archer, of Dublin, have brought to light many very interesting freshwater Protozoa, thus much augmenting our materials for comparison, and adding new zest to inquiry as to their natural affinities or their probable origin and derivatives.

If evolutionary forces are admitted to be in constant operation, it would be hard to say that any two existing forms should stand to each other in the relation of source and product. It would perhaps be safer to say that existing forms have taken their origin from *such* forms as are still in existence ; for as it is but reasonable to suppose that in the lapse of time all the members of the primary type must have undergone some change, the persistence of that type through all in its primitive state is difficult to conceive, though, for any thing we yet know, this may be the case.

Without indulging in this theme further, if we now seek for the most probable derivatives of definite types of Protozoa, some remarkable facts strike us, first, in relation to the Cestoid worms, as bearing upon their possible derivation from the Gregarinidæ. I have already noticed the affinity of the Gregarinidans themselves to *Protomyxa* and *Myxastrum* amongst the *Monera* ; but when we find the hooklets of *Tænia* and the sucker-pits of *Tænia* and *Bothriocephalus* shadowed forth in *Hoplorhynchus* and *Actinocephalus* respectively, we can scarcely help acknowledging the alliance here indicated. In the Gregarinidæ, moreover, there is not only a distinct external integument, but Van Beneden has lately demonstrated the existence of circular muscular fibres on its inner surface ; a similar habit of life in both cases is also very significant. Nor would it be inconsistent to regard the Trematoda and Nematoidea as further developments of the same series of essentially internal parasites.

Now, although the Thalassicollidæ are not parasites, the genus *Thalassicolla* and the *Gregarinæ* alone of all the simple Protozoa take up their nutriment in solution, after the manner of the compound forms, namely the Porifera, restricted Polycystina, and Foraminifera. This fact, I think, is significant, as suggesting the derivation of *Gregarina* from some such original as *Thalassicolla*,

as it does not seem natural to suppose that the former, which is so essentially an Entozoon, could have been descended from a stock capable of assuming solid food in the outer world.

Dr. Carpenter unconsciously gives us the weight of his opinion in the following quotations from his valuable work on the microscope.   On page 449 he says, speaking of *Sphærozoum,* "Towards the inner surface of this (the outer) coat are scattered a great number of oval bodies resembling cells, having a tolerably distinct membraniform wall and a conspicuous round central nucleus, thus corresponding closely with the *Gregarina* type." I might mention in passing that, having frequently taken in the towing-net the unequivocal allies of *Dictyocha* with sarcode bodies identical with those of *Sphærozoum,* I have no hesitation in assuming *Dictyocha* itself to belong rather to the Thalassicollidæ than to the group with which it is more usually associated. This family is commonly included under the head of Rhizopoda ; and there can be no doubt that the generalization, irrespective of that term, is a correct one ; but it is a stretch of transcendental anatomy to speak of the existence of pseudopodia in any member of it.   The radiating branched filaments within the dense external investment of *Thalassicolla nucleata* are not extensions of the sarcode body, like those of *Gromia* for example, but apparently act as retinacula, and as conduits for dialytic currents, which may account for the phenomenon of cyclosis observed in some instances.

Professor James-Clark, of Pennsylvania, appears to have satisfied himself, at least, that there is a remarkable agreement of characters exhibited between the Porifera and the Infusoria, which are connected, as he endeavours to show, by a regular gradation of animals.   The derivation of the latter group of Protozoa from the former, which I had myself assumed quite independently, is therefore supported by that gentleman's researches.

Even with our present advanced knowledge of the Infusoria it is doubtful if we do not still include amongst them the larvæ of *Turbellaria* ; and, indeed, the passage from the one type to the other would appear to be natural and easy.   On the other hand, tracing through such forms as *Nemertes, Bonellia,* and *Priapulus, Sipunculus* will lead directly to the less-equivocal Echinodermata ; and here the series must wind up ; for further evolution, though perhaps possible, does not appear to have taken place.

The existence of such low or simple forms of Rotifera as the genus *Asplanchna,* for example, would be favourable to the idea that the Noctilucidæ might have been the progenitors of that order of beings.   It is of course quite gratuitous, but convenient, at present to assume that the Noctilucidæ would thus hold the same relationship to the Polycystina that the Infusoria appear to do to the Porifera.   However this may be, it is more certain that the Rotifera are at the root of the annulose and articulate series.

From the Rotifera, through the Annelida, we may thus trace the development of the crustaceous and chitinous types of Articulata like a dichotomous branch.

The Annelida may be linked with the Crustacea by means of the Sagittidæ, whose exquisitely striped muscular fibres accord to them a higher position than the other parts of their organization would perhaps warrant them to take.

There is obviously a representative relationship between the crustaceous Macrura, Anomura, and Brachyura and the chitinous Myriopoda, Insecta, and Arachnida.

The earthworms and the leeches may help to fill up the gap between the Chætopod Annelida and the Myriopoda (as, for example, between the genera *Geophilus* and *Nereis*), though it must be confessed that the existing links are inadequate, or they have never been sufficiently made out.

The first rudiments of a tracheal system are probably to be sought for in the Terricolous Annelida, though true articulated limbs and a dorsal heart seem to make their first appearance in the Iulidæ.

Should the simplest hydroid polyps have sprung from such Protozoa as *Difflugia, Arcella*, or *Astrorhiza*, with their pseudopodial tentacula encircling a fixed oral point, the existence of a living series from the lowest type of animals to that which is obviously on the confines of the Vertebrata would be clearly demonstrable[*]. Furthermore, as the interpolation of any other invertebrate types would disturb the harmony here, the inference is natural that they also might be distributed in a similar way into as many groups or series as their affinities or antipathies would suggest or necessitate.

Having studied this subject very carefully, it appears to me that the whole of the Invertebrata admit of distribution into four distinct series, corresponding with the number of sections of the Protozoa, from which all the other types may have taken their origin. Thus, on dividing the Astomatous Protozoa into compound types and their allied simple forms, we obtain the following highly suggestive arrangement, in which the groups represent each other so remarkably that they would seem to be quite natural.

[*] The annexed Table exhibits the progressive modification of the alimentary system in ascending from the Hydrozoa to the Tunicata :—

Evolution of the Alimentary Canal in particular.

MOLLUSCOIDA (including Ctenophora)	Intestine insulated from the somatic cavity ...	With primary hæmal and final neural flexure	*Ascidiozoa.*
		With simple neural flexure.............	*Brachiopoda* and *Polyzoa.*
	Intestine straight, and communicating with the somatic cavity		*Ctenophora.*
CŒLENTERATA	Intestine not yet developed; stomach communicating with the somatic cavity		*Actinozoa.*
	True stomach not yet developed, its office being answered by the somatic cavity		*Hydrozoa.*

Additional matter in the above connexion will be found in a paper by the author " On the Morphological Relationships of the Molluscoida and Cœlenterata," published in the Transactions of the Royal Society of Edinburgh, vol. xxiii. part 3, 1864.

I have appended the Stomatoda and the twelve remaining sections of the Invertebrata in the order indicated by their affinities.

Scheme of Classification of Invertebrata.

Leading Types of Protozoa, aggregate or compound.

1.	2.	3.	4.
Collosphæra.	Porifera.	Polycystina.	Foraminifera.

Corresponding simple forms.

Thalassicolla.	Actinophrys.	Acanthometra.	Amœba.
Gregarina.	Gromia.	Podocyrtis.	Difflugia.

Derivative types.

Cestoidea.	Infusoria.	Noctilucidæ.	Cœlenterata.
Trematoda.	Turbellaria.	Rotifera.	Molluscoida.
Nematoidea.	Sipunculidæ.	Annelida.	Mollusca.
	Echinodermata.	Articulata.	

So as not to complicate the Table, I thought it better to sup- plement it with the definition of the four leading types of compound Protozoa.

1. In the Collosphæra type, the sarcode bodies lie at some distance apart and are always distinct.

2. In the Porifera type the sarcode bodies are closely approxi- mated or confluent.

3. In the Polycystina type the sarcode bodies are concentric and connected by radiating stolons.

4. In the Foraminifera type the sarcode bodies are connected by stolons in linear series or some order of juxtaposition.

If it is incumbent upon the developmental hypothesis to derive the Vertebrata from the preexisting Invertebrata, the only line through which it would be possible to trace their descent is that leading from the Protozoa to the Mollusca proper, or the fourth series of the Table. It would also appear that the Entozoa, Echinodermata, and Articulata appertain severally to separate series of their own : and whatever may happen by-and-by, it would be difficult to find, in the present fauna of the globe, a single form clearly deducible from any of them.

The habit of life of the Entozoon, the peculiarity of structure of the Echinoderm, and the very perfection of organization of the Articulata, as it were, preclude their evolution into any other existing type. To use a common phrase, they may be said to lead nowhere, though they may be easily and, I think, consistently traced back to their possible origin in the Protozoa.

It would be great presumption to say that even an approach to perfection had been attained in this attempted classification of a whole subkingdom of animals. Nevertheless, in the preceding Table, the relationships existing amongst the members of that sub- kingdom are presented to the eye at a single glance, and in a manner that would be quite unattainable by systems maintaining the original creation of every so-called species, and that in an order perhaps more easily described than understood.